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Implicit Learning and Nongrammatical Exposure.

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Implicit Learning and Nongrammatical Exposure

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Abstract

The two experiments presented here are an extension of work done on implicit learning by Arthur S. Reber. Subjects memorized strings of letters during the acquisition phase. Some of the letter strings were created using a finite state grammar (grammatical items), while others were randomly created (nongrammatical items). A total of four different types of acquisition exposure were used in the two experiments: no exposure, all grammatical exposure, all nongrammatical exposure, and mixed exposure (both grammatical and nongrammatical items). After memorizing the acquisition list, subjects were informed that the items they memorized were either rule created, randomly created, or both, depending upon their exposure category. All subjects participated in a discrimination task, in which they were required to identify previously unseen grammatical and nongrammatical items as being either rule created (grammatical) or randomly created (nongrammatical). Analysis of the subjects' discriminatory ability found that subjects who saw all grammatical items during acquisition were significantly better at identifying rule and randomly created items than all other subjects. Since none of the all grammatical exposure subjects were able to verbalize the finite state grammar rules or give an accurate account of their decision processes, it was

presumed that their learning about the grammar was implicit (nonconscious). Subjects in the other exposure groups (nongrammatical, no exposure, mixed exposure) did not reach the performance levels of the grammatical exposure group, but did perform at above chance levels. These subjects were able to make systematic guesses on the discrimination task that enabled them to score at significant levels, possibly by using their prior knowledge of probability rules and randomness. Subjects in the mixed exposure groups, while exposed to some grammatical items during acquisition, did not seem to be able to utilize the grammatical information in any consistent way when making discriminations. This could be due either to a dearth of grammatical instances for study, or to confusion caused by the inclusion of nongrammatical instances which might have been treated as valid grammatical instances. Because subjects can utilize prior knowledge when discriminating items, control groups are essential for setting the true guessing baselines in implicit learning experiments.

Introduction

Human beings are capable of learning extremely complex and complicated material. Paradoxically, they are not always capable of explaining or identifying this learning process. In some cases, the individual is completely unaware that learning has taken place. In order to capture the learning process, an experimenter must ask not only what is being learned, but also how it is being learned.

The purpose of the current experiments was to further elaborate on work on implicit (nonconscious) learning done by Arthur S. Reber (Allen & Reber, 1980; Kassin & Reber, 1979; Reber, 1967, 1969, 1976; Reber & Allen, 1978; Reber, Kassin, Lewis & Cantor, 1980; Reber & Lewis, 1977). Reber's experiments consisted of several phases, the first of which was an acquisition phase wherein subjects memorized items generated by a grammar. Subjects then took a discrimination test and tried to identify previously unseen items as grammatical or nongrammatical. These experiments did not contain control groups, leaving open the possibility that subjects could be learning during the testing phase of the experiment rather than during acquisition. In addition, the lack of proper control groups obscures the extent to which subject behavior on the test is due to previously learned information and heuristics rather than to information acquired during acquisition. Control subjects who do not experience grammatical exposure during acquisiton are necessary in order to isolate the extent events outside of the acquisition phase contribute to behavior which could be misconstrued as evidence of implicit learning. This is done in Experiment 1 of this paper.

Another area of concern in Reber's experiments is the fact that the acquisition set contains only grammatically correct items. It is very rare in real-life to learn anything without the inclusion of some malformed or erroneous instances; children learning the concept of "furniture", for example, have to deal with the mail on the table and the tricycle on the floor which are not pieces of furniture, but have some "furniture" characteristics (you can sit on a tricycle). Thus Reber's stimulus environment may be so special that it allows for a type of learning behavior not found under more typical circumstances; seeing only items belonging to a category may allow subjects to easily discriminate that category. Having to isolate the category definition when both category instances and noninstances are present may prove more difficult. On the other hand, noninstances could provide additional information about the limits of the category, and thus could improve discriminatory performance. The effect of category noninstances on implicit learning is explored in the second experiment.

Reber's Implicit Abstraction Paradigm

In order to provide background for the two studies presented in this paper, a history of Reber's work to date on implicit learning will first be presented. Studies which refute and/or support his findings will be reviewed. An overview of Reber's implicit abstraction model is also presented. Implicit learning does not fit neatly into the framework of more traditional learning theories; Reber assumes that he is dealing with analytical, nonconscious learning.

All of Reber's experiments concern the learning of artificial languages. These languages are generated by using finite state grammars (FSGs). An example of one such

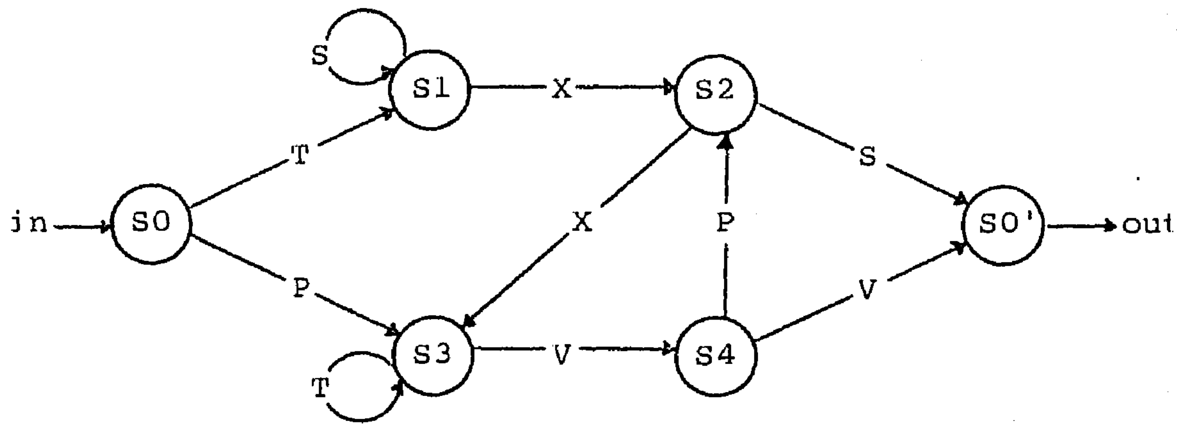

Figure 1. Finite state grammar used to generate stimuli. Each transition from one state to another state corresponds to a rule (Reber, 1967).

grammar is in Figure 1. Grammatical strings are generated by any series of state transitions from the initial state (S0) to the exit state (S0'). A string is generated in a fixed order, from past state to future state, and the resultant series of letters is written from left to right with the rightmost letter being the one most recently generated (Chomsky & Miller, 1958). For example, the state sequence 0-1-1-2-3-4-0' for the grammar in Figure 1 would generate the string TSXXVV. Note that while this grammar embodies a finite set of rules (state transitions), it can generate an infinite number of strings of length 3 to infinity. When working with FSGs, most experimenters arbitrarily set a length limit for stimulus items, and then use the FSG to generate all possible items of the required length.

Reber chose to use FSGs in his experiments because they generate complex stimulus items of a probabilistic nature (Reber, 1967). For example, for the set of all possible strings from the grammar shown in Figure 1, the probability is 0.5 that an item will begin with P, and there is a 0.25 probability that the second letter will be a T. These probabilistic state interrelationships bias against an existing encoding strategy that would allow subjects to use conscious mnemonics successfully (Reber, 1976). In real-life, many complex stimuli have contingencies which are not immediately discernible but which are highly probabilistic in nature, such as weather patterns and spelling (Reber & Allen, 1978). Unfortunately, the rules governing the creation of naturally occurring complex stimuli are often hard to specify. Finite state grammars are useful in experimental settings because they generate complex stimuli with probabilistic intra-relationships, and have the added benefit that the experimenter can completely specify all of the generative rules for the grammar.

In his earliest experiment, Reber (1967) had subjects memorize either grammatical strings or randomly formed strings. Subjects were not told about the possible existence of a rule system (grammar) until after they had successfully memorized the strings, which were presented in 7 sets of 4 strings per set. The four strings in a set were presented one at a time in sequence, after which the subject tried to reproduce them. This process was repeated for each set of four strings until the subject could correctly reproduce the strings on two consecutive occasions.

Analysis of the learning patterns for subjects learning grammatical strings versus those learning randomly formed (nongrammatical) strings showed that both groups had a decrease in errors from the first to second set. In addition, the grammatical string subjects continued to show a decline in number of errors across the remaining sets. Reber hypothesized that after the first set, subjects selected a systematic memorization strategy (one item at a time) rather than trying to retain all 4 items at once. This accounts for the initial decrease in errors for both groups. Since grammatical exposure subject performance continued to improve beyond that of the nongrammatical exposure subjects, the grammatical exposure subjects must have been using information or organization techniques unavailable to the nongrammatical (randomly formed items) exposure subjects. However, inquiries of the grammatical exposure subjects did not elicit any verbal knowledge of the FSG rules, even with prompting.

Reber (1967) designed a second experiment to determine if the grammatical exposure subjects did indeed "know" the FSG rules. Subjects learned grammatical items as before, and then were told about the existence of a set of rules; they were not told the exact nature of the rules. The subjects were then given a list of 44 new strings, half of which were previously unseen grammatical items. The remaining strings were nongrammatical items, some with very subtle errors (one letter FSG violations). Subjects classified these strings into grammatical and nongrammatical categories at a rate well above chance (an average of 69% correct). String length did not account for this behavior, since all grammatical items were called nongrammatical at about the same rate. Subjects were again unable to verbalize any rules used in making their decisions. Because the subjects' discriminatory behavior indicated they did indeed have knowledge about what constituted grammatical items, their continued inability to verbalize this knowledge led Reber to postulate a nonconscious (implicit) learning process. During the memorization phase of the experiment, subjects seemed to be implicitly (nonconsciously) absorbing details of the FSG rule structure, which enabled them to decrease the rate of memorization errors and to make grammatical versus nongrammatical distinctions in the test phase. Reber hypothesized that subjects were not consciously analyzing the memorization strings, and were not even aware of the existence of the rules they were "using" to make decisions about the test strings.

Reber and Lewis (1977) repeated the above experiment with similar results. After having memorized FSG generated items, subjects were capable of discriminating new grammatical items from nongrammatical items (single letter FSG violations) at a mean rate of 80% correct, regardless of item length. Subjects also solved anagram puzzles by rearranging a scrambled set of letters into an FSG acceptable order. Performance on this task improved significantly over trials despite the absence of feedback on correctness. In post-experimental essays, subjects confidently defined rules which were totally inconsistent with their performance on both the discrimination and anagram tasks. The fact that subjects were not consciously aware of the true regularity of the system nor of their own correspondingly regular behavior provided further evidence for the existence of nonconscious learning. Reber (1967, 1969) defined implicit learning as an analytical process that operates at a nonconscious level to extract regularities from the environmental stimuli. The result of implicit learning is an abstract internal representation of the underlying rule system which can be used in dealing with new stimuli.

Results remarkably consistent with Reber's findings were found using FSG patterns of nonspeech sounds (Howard & Ballas, 1980). Subjects who tried to learn FSG generated sound patterns improved their retention rate with practice, but subjects who tried to learn randomly generated patterns did not. In addition, the FSG pattern subjects were significantly better at discriminating FSG items from noise (randomly created) items. Thus implicit learning of the underlying generative rules can occur with both visual and aural stimuli, and does not require a language environment (letters); implicit abstraction still occurs with stimuli composed of nonspeech sounds and tones.

Implicit Learning in Other Paradigms

While not directly related to Reber's work, the following studies also seem to demonstrate the existence of a nonconscious implicit abstraction process. Jones (1973) performed a serial learning task in which subjects were asked to learn numerical items consisting of 24 digits. Each item was presented for study 8 times in succession. After each of the 8 study trials, the subject tried to write down as many of the digits as possible. Unbeknownst to the subjects, some of the items were sequentially organized (e.g. 112233665544223344554433) so that the order of subsequent digits depended upon the order of the preceding digits. Other items were randomly organized, with no consistent relationships between the digits. Subjects were able to exploit the pattern symmetries when learning items with a sequential organization, resulting in a reduced error rate over trials relative to performance on the non-ordered (randomly organized) items. The sequentially organized stimuli met Reber's requirements for nonconscious learning by being both complex and probabilistic in nature. While Jones did not question the subjects to determine the degree of conscious versus nonconscious analysis, she felt that the subjects, who had no previous knowledge that a rule system for organization existed, were forming an implicit abstraction of the rules which could be used to aide subsequent learning.

A later serial learning study by Marmurek and Johnson (1978) required subjects to learn permutations of a base sequence of letters. Learning took place during an anticipation paired associate task. On each trial, subjects first saw a base sequence of letters followed by a symbol. They were instructed to call out the permutation sequence they expected to see, and were then shown the correct permutation for a brief study period. Some of these permutations were consistently organized, so that a given letter was always regrouped in conjunction with other letters and was not separated from them by intervening letters. Other permutations were randomly organized, with no restrictions on letter regroupings. Despite controlling for the frequency of adjacent sets of letters in both permutation types, the consistently organized permutations resulted in significantly fewer anticipation errors overall. While subjects were not questioned about their strategies, it was again assumed that the probabilistic interrelationships of the consistent stimuli enabled subjects to implicitly pick up on the permutation consistencies and improve their guessing accordingly. In both of the previous serial learning experiments, subjects were not informed about the existence of rules, but were still capable of implicitly using the existing organizational information as an aid in learning and discriminating items.

While it has thus been demonstrated that learning can take place when subjects are not aware that rules or stimulus patterns exist, it is still not clear from the above studies whether conscious search strategies are also involved in what has been called "implicit learning." Baron and Hodge (1978) attempted to suppress conscious searching for rules by informing their subjects that no rules or interrelationships among stimulus items existed. It was assumed that subjects would not bother investing time in a conscious analysis of the stimuli which could not be expected to impart any worthwhile information. In the

Baron and Hodge experiment, subjects learned to pronounce nonsense words written in an artificial alphabet. While subjects were told that no correspondences existed between letters and sounds, half of the stimulus items did possess correspondences, with certain letters consistently associated with specific sounds. The correspondences were from right-to-left, so that the rightmost letter was associated with the initial sound of the word and the leftmost letter with the terminal sound. The other words were randomly created so that letters and sounds did not correspond to each other in any consistent way. Subjects were given 25 runs through the list of letter-sound pairs, with the subject attempting to anticipate the correct pronunciation for each word. Subjects made significantly more pronunciation errors when learning the random items than when learning the correspondence items. When given a transfer task involving pronouncing a previously unseen correspondence item, subjects performed significantly above chance.

In order to test the robustness of this result, the correspondence rule (right-to-left) was identified for the subjects and they repeated the transfer task. There was no significant increase in performance. Explicit knowledge of the correspondence rule thus did not add to the subjects ability to decode and pronounce items; they had already implicitly learned a comparable amount of knowledge about the correspondences between the letters and sounds. Subjects were able to extract the letter-sound regularities which applied to half of the words without the aid of explicit instruction and despite verbal evidence that no such regularities existed. It appears that implicit learning may operate effectively despite conscious expectations that there are no rules to be learned.

While the kinds of rules used in the preceding experiments were very different (from FSGs to right->left correspondences), all of them impose a degree of regularity on otherwise meaningless stimuli. Some kind of constant covariation between stimulus items seems critical in order for implicit learning to take place (Nagata, 1981). It could be argued that all that is required in implicit abstraction is recognition of regularity versus irregularity (randomness). Kantowitz (1971, in Britton, 1980) tested this possibility by training subjects on FSG generated strings, followed by a discrimination task using new strings from the original FSG (grammatical items) and strings from a second FSG (nongrammatical items). Subjects were able to identify grammatical items at a rate well above chance. Since the nongrammatical items were also ordered stimuli, subjects should not have been able to distinguish grammatical items from nongrammatical items simply by recognizing "regularity". Of course, since this study did not possess a control group, it must be assumed that the two FSGs were so similar that uninformed subjects (with no acquisition experience) could not easily distinguish them. If the two FSGs were indeed sufficiently alike, then correct discrimination must haved involved more detailed knowledge of the underlying grammatical rules.

Implicit versus Explicit Learning

As Reber (1967) and Reber and Lewis (1977) have shown, detailed knowledge of the underlying rules does not appear to be verbalizable by the subjects. However, nonverbalizable and implicit (nonconscious) learning are not necessarily synonymous. In an attempt to clarify the differences between explicit and implicit learning, Reber (1976) repeated his learning-discrimination study (Reber, 1967) using both informed and uninformed subjects. The uninformed subjects (implicit group) were instructed simply to memorize the FSG generated acquisition strings, while the informed subjects (explicit group) were told that discovering the rules would make memorization easier. Results showed that explicitly searching for rules actually made learning more difficult; the explicit group took significantly longer to memorize the items. The explicit group was also significantly poorer than the implicit group at discriminating grammatical items from nongrammatical ones, though both groups performed at an above chance level.

In order to test the consistency of the responses, the items in the discrimination test were repeated. Responses to the same item can be construed to be based upon some decision strategy or knowledge if the number of grammatical-grammatical and nongrammatical-nongrammatical responses is significantly above chance expectations. The implicit group had a significantly high incidence of correct-correct responses, indicating the stability of their "knowledge". The explicit group's knowledge was stable, but inaccurate, as evidenced by a high double error rate. The implicit group's double correct rate was approximately equal to the explicit group's double error rate, indicating that while both groups apparently learned the same amount, what the explicit group learned was incorrect. It appears that the explicit rule-search process masked the implicit process, leading subjects to consciously discover erroneous rules and possibly suppress the nonconscious abstraction of the correct rules.

Once again, experiments using different learning paradigms support these results on the differences between explicit rule search and implicit abstraction. Using a serial learning format, Jones (1973) instructed subjects to anticipate the next number in a sequence of 24 digits. These subjects took much longer to learn the sequence than subjects who were told simply to study the numbers as they appeared. Baron (1975) had subjects memorize paired associate lists after being explicitly told the association rules that related the stimuli to their responses. These subjects took longer and made more errors than subjects who were merely told to memorize the lists. In both experiments, explicit attempts to use or discover rules slowed subjects down and increased their recall error rate relative to subjects who merely observed or memorized the stimuli. Thus conscious knowledge of the existence of rules is not only unnecessary for implicit learning (Baron & Hodge, 1978), it can be detrimental to nonconscious learning.

Subjects need not be told about the existence of the rules in order for conscious processes to intrude upon effective implicit learning, however. In an experiment by Howard and Ballas (1980), subjects experienced increased difficulty in learning FSG generated nonspeech sound sequences when the sounds were real-world noises instead of pure tones. Learning real-world sound sequences also resulted in poorer discrimination of grammatical and nongrammatical sequences than did learning tones. The subjects reported that being able to identify the sounds led to confusion when they tried to link typically unrelated sounds together in the sequences. The nature of the real-world stimuli prompted explicit "search and identify" activities that interfered with implicit absorption of the underlying rule structure.

In order for implicit learning to occur, then, it is necessary for the stimuli to possess regularities of a sufficiently complex and unfamiliar nature that subjects do not try to consciously induce the underlying rules (Brooks, 1978; Reber, et al., 1980). If explicit rule search is to be effective, the stimulus regularities must be salient to the subject. Reber, et al. (1980) attempted to make the underlying FSG structure more salient by mounting the acquisition strings on a board in a systematic order (salient board). Each column of strings represented a different subset of grammatical items: those beginning with T and ending with VV, those beginning with P and ending with VV, etc. A second board was prepared with the strings mounted in random order (random board). Subjects were given one of the two boards and told either to memorize the strings (implicit group) or to figure out the rules and use them to help remember the strings (explicit group). All subjects then took the discrimination test of previously unseen grammatical strings and nongrammatical strings (single letter FSG violations). Results showed a strong salience by instruction interaction, with the explicit group subjects performing significantly better than all other subjects on the discrimination task if they worked with the saliently organized board, but significantly more poorly than all other subjects if they used the randomly organized board to memorize the acquisition items. It appears that explicit rule search can be effective on complex, probabilistic stimuli if the intra-relationships in the stimuli are available to the subject; the nature of the presentation (salient board) reduced the complexity of the stimuli to a level where subjects did not need to group the patterns into like sets and could simply analyze the presented groupings.

To check on response consistency, the items in the discrimination test were repeated. The highest double-error rate was found in the random board, explicit instruction subjects, indicating that their active search had led to discovering invalid rules. The highest double-correct rate occurred in the explicit instruction, salient board subjects, presumably because they learned more rules.

An Analog Interpretation

It could also be argued that the salient board organization in the Reber et al. experiment (1980) provided explicit instruction subjects with a better memory chunking system for the acquisition strings. Their superior performance on the discrimination task would then be due not to knowing more rules but to retaining and being able to access more grammatical acquisition strings. These strings could be compared with a new string to determine its degree of similarity to grammatical items (Britton, 1980; Brooks, 1978); no knowledge of the grammar rules would be necessary.

This latter theory proposes that subjects are determining the grammatical or nongrammatical nature of discrimination strings by means of analogy to remembered grammatical strings. This is a nonanalytic process requiring storage of individual instances. The separate aspects of the stimulus are not broken down and stored as rules or laws; the entire stimulus item is retained. The analogy theory thus requires only storage of the items, and does not require processing during acquisition. Reber's implicit abstraction theory requires that the items be analyzed nonconsciously during acquisition, and the individual items are not retained. According to implicit abstraction, a new item is classified by comparison to the remembered rules. Under the analogy theory, the similarities between the new item and stored exemplars are analyzed, and if the match is close enough, the item will be classified as being of the same type as the exemplars. One problem with the analogy theory is that a new item could be misclassified if there were no exemplars in memory analogous to it. The analogy theory does allow greater flexibility should category requirements change, since the stored exemplars need only be reshuffled; under implicit abstraction the entire abstract rule system would have to be reworked.

Brooks (1978) performed a paired associate task which demonstrates implicit learning by analogy. Two paired associate lists of letter string stimuli were made up using two FSGs. The responses were all either cities or animals, but this obvious division was not what distinguished the two grammars. Grammar A items were associated with "old world" responses (such as Rome and elephant), and grammar B items with "new world" responses (such as Chicago and buffalo). After learning the responses to both lists (in random order), subjects were told about the new/old world response distinction and then took a discrimination test in which they sorted new strings into three piles. One third of the discrimination strings were "old world" strings (grammar A), one third were "new world" strings (grammar B), and one third were strings acceptable by neither FSG. Subjects could discriminate grammars A and B from each other and from the nongrammatical strings at a rate significantly above chance (an average of 60% correct).

Using control groups, Brooks showed the two FSGs were not distinguishable without consistently correlated responses. Since subjects did not know that there were two types of items during acquisition (except for the obvious but erroneous city/animal response distinction), it was unlikely that they were correctly extracting the two different FSG rule structures. To emphasize this point, Brooks repeated the experiment, this time associating grammar A items with city responses and grammar B items with animal responses. If subjects develop rules systems, having more obvious categories should result in greater ability to learn the rules. In reality, subjects performed at a level that was not significantly different from the previous experiment. Since increasing the salience of the category distinction did not increase the amount of learning, implicit abstraction of the underlying rule system may not be necessary for significant discriminatory ability in a paired associate learning task. Because the obscure new/old world response organization provided results similar to the more obvious animal/city organization, it is clear that response regularity need not be obvious to the subject during the learning phase. Brooks proposes that subjects were memorizing the individual acquisition items, and using these individual instances to draw analogies with the discrimination items. In this way, subjects need not know the response categories at acquisition. Once informed about the relevant categories, subjects would be able to retrieve memory items analogous to test items and interpret the exemplar item responses to predict a category for the test item.

Brooks also points out that subjects need not have perfect memory for individual instances. If a subject retrieves from memory an item that resembles the test item but is incomplete, his/her chances for correct categorization depend on whether the forgotten data is relevant or irrelevant. If a retrieved item has four dimensions and the value of one of them is unknown, the subject still has a 75% chance of having retrieved an item that is correctly analogous to the test item. The limiting factor on analogical comparisons is having enough stored representative instances which are as complete as possible; perfect memory is not a requirement for success above chance levels.

Analogy versus Implicit Abstraction

Unfortunately, because Brooks' analogy theory is based upon a different experimental learning paradigm than Reber's implicit abstraction theory (paired associate learning versus memorization learning), it is not clear whether these theories are describing the same implicit learning phenomena. Several studies have been performed in an effort to discriminate between the analogy and implicit abstraction processes.

Baron and Hodge (1978) performed one such experiment using spelling-sound correspondences. Subjects learned to pronounce nonsense words written in an artificial alphabet. One set of acquisition words was consistent, with one letter corresponding to only one phoneme throughout the set. Another acquisition set was inconsistent, with a given letter having different sounds in different words. Both sets had similar letter stimuli and sound responses, and were constructed so that all responses in a set had at least one phoneme-letter combination in common. The analogy theory would predict the consistent phoneme-letter pattern could lead to high transfer performance despite inconsistencies in the set, since subjects could simply search for items with analogous letters. The analogy theory thus would predict that the two sets should result in equal amounts of learning, as evidenced by the ability to pronounce a new word correctly. Implicit rule abstraction predicts the consistent set would result in more accurate rule learning and greater transfer accuracy; consistency is a requirement for accurate rule abstraction (Nagata, 1981). Results showed no significant differences in ability to pronounce new words between the consistent and inconsistent acquisition groups, in contradiction to the implicit abstraction theory.

The analogy versus implicit abstraction argument was also studied in work done on fuzzy categories by Medin and Schaffer (1978) and Medin and Smith (1981). Two categories were constructed so that no one item feature, such as color or shape, was associated strictly with any one category. Items within a category were constructed to be more analogous to each other than to items in the other category. However, all items were the same in terms of average similarity to either category prototype. It was assumed in this experiment that implicit abstraction results in a set of rules for which the category prototype is the best possible example. Subjects were first required to learn an acquisition set of items from both categories. As each item was shown, subjects responded by guessing that the item belonged in either Category A or Category B. The correct category was then identified. The list of items was presented until the subject met the criterion of one errorless trial, or until a maximum of 32 trials was reached. The subject then sorted the discrimination items into two groups. The prototype model (adding distinctive features) would predict random category assignments because all items are equidistant from both prototypes. The analogy model predicts specific category assignments based on the number of analogous stored exemplars; the category with the greatest number of remembered exemplars analogous to the stimulus item will be the category chosen. Results showed greatest support for the analogy model of exemplar comparison.

Subjects were also asked to identify the discrimination items as new (not seen before) or old (seen during acquisition). The analogy model predicts that the greater the number of stored exemplars analogous to a new stimulus item, the more likely it will be falsely classified as an old item. Prototype theory predicts erroneous classification on the basis of closeness to the prototype; since all items are equally close to all prototypes, they should all have the same probability of being falsely recognized as old items. Results again showed significantly dissimilar responses to individual items, supporting the analogy theory of stored exemplars in decision making.

All three of the above experiments (Baron & Hodge, 1978; Medin & Schaffer, 1978; Medin & Smith, 1981) seem to lend credence to the analogy theory. Reber, however, does not agree that the analogy theory is completely applicable to all of implicit learning. When subjects attempted to solve FSG anagrams (Reber & Lewis 1977), the best predictor of performance was a model based on the frequency of letter pairs (bigrams) occurring in the entire set of grammatical strings. The bigram frequency pattern of the acquisition string subset did not correlate with performance at all. Since the acquisition strings composed the subjects' potential analogy set, it is plain that simple rote memory for items does not account for performance which was correlated with knowledge of the entire FSG's bigram frequencies.

Similarly, an experiment by Reber (1969), in which he manipulated FSG syntax and symbols, points strongly towards rule system knowledge rather than exemplar knowledge. After memorizing an acquisition set, subjects were asked to learn a new set of strings which differed from the original set either in syntax (new FSG) or symbols (new letters, same FSG). If subjects rely on exemplar storage to detect consistencies, either a symbol change or a syntax change should produce a comparable number of errors since new strings would have little in common on the surface with the acquisition items. If subjects abstract at least part of the underlying rule structure, then they should recognize the consistency in the rules despite a surface symbol change, and should produce fewer errors when learning the new set of items. The results showed that subjects made significantly fewer errors when learning the new symbol, same FSG items than when learning the new FSG items. In addition, the symbol change subjects quickly decreased their error rate to their lowest acquisition error rate. These subjects were able to exploit their knowledge of the rules from the acquisition set to speed learning of the new set of items.

Paired Associate versus Observational Learning

The fact that support can be found for either theory (implicit abstraction or analogy) can be traced to the learning paradigms used in the experiments. Results supporting the analogy theory come from paired associate learning studies (Baron & Hodge, 1978; Medin & Schaffer, 1978; Medin & Smith, 1981), whereas results supporting the implicit abstraction theory come from memorization and/or observational learning studies (Reber, 1969; Reber & Lewis, 1977). The inclusion of responses and/or response feedback during acquisition seems to effect the type of learning involved. Reber and Allen (1978) performed an experiment designed to compare the degree and types of learning resulting from the paired associate and observational paradigms. Subjects learned FSG items either by "attending to them" (implicit abstraction), or by learning the city responses paired with the strings (paired associate learning). The implicit abstraction subjects were able to decrease their recall error rate over learning trials, but the paired associate subjects did not decrease their errors and some subjects even increased the number of recall errors as the number of items to be remembered increased over trials. The two groups reported very different processing styles during the acquisition phase. Paired associate subjects admitted to using gimmicks and mnemonics when remembering the city-string pairings. In contrast, the implicit abstraction subjects claimed to be using more wholistic scanning/acoustical methods.

The true test of the two reported learning methods came after acquisition. Paired associate subjects were asked to identify the discrimination strings as allowable "cities" or "not cities", and the implicit abstraction subjects judged the well-formedness (grammatical or nongrammatical) of the strings. Despite the fact that the paired associate subjects received two times more exposure to the acquisition strings than did the implicit abstraction subjects, the latter performed significantly better at identifying grammatical items on the discrimination task. Paired associate subject performance, while above chance levels on the average, decreased significantly during the discrimination test, possibly due to the decay of analogy exemplars from short term memory. If stored exemplars do indeed have a limited life span, then subjects who learn by the analogy method should be at a disadvantage as time passes by and they forget what they have learned.

The life span of exemplar memory was tested in a two year follow-up study by Allen and Reber (1980). Subjects from their 1978 study repeated the discrimination task without any review of the original acquisition items. The discrimination items consisted of both novel (previously unseen) and old (seen in acquisition) strings. While both groups exhibited decreased performance from their original levels, they were still above chance in their ability to identify grammatical and nongrammatical strings (an average of 67% correct). There was no overall scoring difference between the paired associate and implicit abstraction subjects, but the implicit abstraction subjects in general were more accurate on the novel items, and the paired associate subjects were more accurate on the old items. These differences were significant.

The two year follow-up results seem to indicate that implicit abstraction subjects processed the acquisition set at a deeper, more global level than did the paired associate subjects, who were mainly trying to establish memory links between items. Implicit abstraction seems to aid subjects in identifying novel grammatical strings by comparison to abstract rules, while the paired associate storage of exemplars during acquisition gives subjects an advantage on identifying previously seen items (Allen & Reber, 1980; Matsuda & Robbins, 1977). Both methods resulted in implicit learning: there was no explicit analysis, subjects could not verbalize how they knew an item was grammatical, and performance indicated accurate knowledge of the stimulus structure (Reber & Allen, 1978). Furthermore, both forms of implicit learning (implicit abstraction and analogy) were robust enough to allow above chance performance two years later, despite the lack of rehearsal opportunity.

Implicit learning has thus been shown to occur under a variety of conditions, from active memorization (Brooks, 1978; Jones, 1977; Reber, 1967, 1969; Reber & Allen, 1978) to simple observation (Reber & Allen, 1978). The differences in type and degree of learning seem to be keyed to the structure of the task required of the subjects (Simon, 1975). The task organization provided by the experimenter (paired associate learning versus observation) may predispose subjects toward particular learning strategies (Martin & Noreen, 1974; Matsuda & Robbins, 1977; Medin & Smith, 1981). On the other hand, the nature of the task may simply make certain aspects of the stimuli more salient to subjects: paired associate learning may emphasize symbols, observational learning may emphasize patterns (Weiner-Ehrlich, Bart, & Millward, 1980; Winston, 1978). Experience with the learning paradigm may also play a significant role. It could be argued that subjects have had much more experience with observational learning (a process that could be operating almost continuously in daily life) than with paired associate learning. The more experience one has with a particular learning paradigm, the more likely it may be that one is capable of extracting the majority of the information as quickly as possible (implicit abstraction). Less experience with the paradigm may require a more careful study of individual items, predisposing subjects to store individual exemplars.

Experiment 1

Because Reber's observational/memorization learning paradigm is the only experimental paradigm to provide results strongly in support of the implicit abstraction theory rather than the analogy theory, it is important to ascertain that subject behavior under this paradigm is not due solely to learning occurring outside of the acquisition phase. Experiment 1 introduces control groups into Reber's learning paradigm for this reason. Reber did not use control groups, instead he relied on chance levels of performance as his baseline. Since subjects could classify the discrimination items as grammatical or nongrammatical, the probability that they would be correct "by chance" was 0.50. This strategy presumed that subjects brought no knowledge to the experiment that would enable them to make systematic and potentially accurate guesses on the discrimination task. In Experiment 1, the validity of this assumption was tested by using two control groups: only nongrammatical (random) acquisition exposure and no acquisition exposure. The performance of these control subjects on the discrimination task should provide information about the true level of correct responses possible by uninformed subjects. This level might be above or below Reber's baseline level of 50% (chance).

Method

Fifty-one male subjects from the introductory psychology subject pool were randomly assigned to one of three exposure (acquisition) groups: grammatical, nongrammatical, or no exposure. Acquisition items for the grammatical exposure group were derived from the FSG in Figure 1. This is the same grammar used by Reber (1967, Reber & Lewis, 1977). Acceptable grammatical items consisted of all strings of length 3-8. Fifteen grammatical strings which sample all of the possible FSG paths (the same 15 used by Reber & Lewis, 1977) composed the grammatical exposure acquisition set. The grammatical items were presented in a different random order for each grammatical exposure subject.

The fifteen nongrammatical strings of length 3-8 for the nongrammatical exposure acquisition set were randomly generated using the same letters as in the FSG. There were no restrictions on the random items except that they could not be grammatical. Each nongrammatical exposure subject viewed a different random order of the nongrammatical items.

Grammatical and nongrammatical subjects were informed that they were part of a memory experiment, and would be memorizing 15 items. The strings were presented in 5 sets of 3 strings each. The 3 strings in each set were presented one at a time on a CRT screen (cathode ray tube computer terminal) for 5 seconds each. After viewing all 3 strings, subjects were asked to type in the strings via a keypad. Subjects could enter the strings in any order, and were informed for each string whether they were correct or incorrect, but were not told the nature of the error. If the subject did not correctly remember all 3 strings, the set was presented again as before and the subject attempted to correctly enter the strings. This process was repeated until the subject correctly entered all 3 strings in a set during the same trial. The subject then proceeded to the next set of 3 strings. The no exposure subjects did not participate in the acquisition phase of the experiment.

All subjects were then informed about the existence of a rule system which would specify allowable letter patterns, but were not told the exact nature of the rules. Grammatical and nongrammatical exposure subjects were told they had learned rule/random items respectively. All subjects then were told about the discrimination items: half would be previously unseen grammatical strings, and half would be previously unseen nongrammatical (random) strings. The discrimination set consisted of 28 acceptable grammatical strings not used in acquisition and 28 randomly created nongrammatical strings also not used in acquisition. The strings were presented in a different random order for each subject on a CRT screen. Only one string was visible at any given time. For each string, subjects were instructed to press a button marked "YES" if the string was considered to be grammatical (follows rules), or to press a button marked "NO" for nongrammatical (random) strings. Response latency was not timed.

Results

Discrimination responses of "YES" and "NO" by the subjects were scored by assigning a value of 1 (one) for each correct response (grammatical item and "YES" response or nongrammatical item and "NO" response) and a value of 0 (zero) for each incorrect response. The total percent correct on the two types of items (grammatical and nongrammatical categories) for each exposure type (grammatical, nongrammatical and no exposure) is shown in Table 1. The nongrammatical and no exposure groups performed at similar levels on the discrimination task, and correctly identified approximately the same number of grammatical and nongrammatical items. The grammatical subjects were better at identifying the grammatical items than the nongrammatical items, and were better overall at the discrimination task than either of the two control groups.

Table 1
Percent Correct Scores by Category
With Grammatical, Nongrammatical and No Exposure

	Exposure		
Category	Grammatical	Nongrammatical	No
Grammatical	82	56	61
Nongrammatical	70	58	58

Planned comparisons were done on the exposure groups and the exposure by category interaction. While the two control groups (nongrammatical and no exposure) did not significantly differ from each other ($F(1,48)=0.60$, $p>.05$), the grammatical exposure group was significantly different from both of the controls ($F(1,48)=41.68$, $p<.01$). The exposure by category interaction was also significant for the grammatical exposure group versus the two controls ($F(1,48)=6.32$, $p<.05$), and was not significant when the two controls were compared ($F(1,48)=0.54$, $p>.05$). All of the individual cell averages in Table 1 were significantly above chance levels according to t-tests. An analysis of variance on the data (see Appendix A for the model, mean squares and ANOVA summary table) showed a significant item effect ($F(54,2592)=4.07$, $p<.01$) and a significant exposure by item interaction ($F(108,2592)=1.42$, $p<.05$).

A second analysis of variance was performed, assuming that the individual grammatical and nongrammatical items were random rather than fixed factors (see Appendix B for the model, mean squares and ANOVA summary table). The purpose of this assumption was for prediction and generalization purposes; it is acknowledged that in this design the items were indeed fixed factors. Comparisons on the exposure and exposure by category effects using this model resulted in quasi-F's. Only the comparison of grammatical exposure versus the two control groups (nongrammatical and no exposure) was significant ($F(1,107)=26.12$, $p<.01$). Both items and subjects were significant effects under this analysis ($F(54,2592)=4.07$ and $F(48,2592)=2.32$ respectively, both $p<.01$). Also significant were the exposure by items and category by subjects interactions ($F(108,2592)=1.42$, $p<.05$ and $F(48,2592)=1.76$, $p<.01$ respectively).

Data from the acquisition phase were compared for the grammatical and nongrammatical exposure groups. No differences were found between the two groups on the number of trials per set needed to meet criterion. The main effect for sets was the only significant effect ($F(4,128)=4.15$, $p<.01$). Comparisons for linear and curvilinear trends showed this to be due to a strong linear

Table 2
Mean Number of Trials per Acquisition Set

Sets 1	2	3	4	5
5.18	4.94	4.15	3.12	3.41

decrease over sets in number of trials to criterion, as can be seen in Table 2 ($F(1,128)=14.46$, $p<.01$ for linear, $F(3,128)=0.71$, $p>.05$ for curvilinear).

Experiment 2

Results from Experiment 1 show that implicit learning does indeed take place. Grammatical exposure subjects were significantly better on the discrimination task than the controls. However, the results also demonstrate that chance is not an accurate baseline when determining the extent of implicit learning during acquisition; control group subjects, who had no exposure to the FSG during acquisition, performed significantly above chance when discriminating grammatical and nongrammatical items. The control group subjects appear able to make systematic guesses about the discrimination items, despite their presumed lack of rule knowledge and experience. Experiment 2 was designed to further explore the effects of noninstances (nongrammatical items) seen during acquisition on discriminatory strategies, as well as the effect of total nongrammatical exposure during acquisition on discrimination performance. Two new types of acquisition exposure were used in addition to the all grammatical and all nongrammatical exposure groups from Experiment 1: an acquisition set with predominantly grammatical items and a few nongrammatical items, and a set with predominantly nongrammatical items and a few grammatical items. It was hypothesized that a mixture of grammatical and nongrammatical acquisition items might interfere with rule abstraction due to the inconsistencies between items in the acquisition set. Subjects viewing a mixed acquisition set were expected to demonstrate discriminatory ability somewhere between that of the control group (all nongrammatical exposure) and the all grammatical exposure group.

Method

Forty-eight male subjects from the introductory psychology subject pool were randomly assigned to one of four exposure (acquisition) groups: 100%, 80%, 40%, and 0%. The percentages indicate the proportion of the 15 grammatical acquisition strings from Experiment 1 seen by the subject during the acquisition phase. Fifteen nongrammatical strings were randomly created using the same letters as in the FSG, and were matched to the grammatical strings in terms of proportion of items of length 3-8. Subjects saw (100-x%) of these nongrammatical strings, where x indicates the amount of grammatical exposure. Thus, the 100% group saw all grammatical items, and the 0% group saw all nongrammatical items. A different random combination of grammatical and nongrammatical strings conforming to the required percentages was selected for each subject in the 80% and 40% groups. The acquisition sets were presented exactly as in Experiment 1, using the criterion of one correct reproduction of all 3 strings in a set during the same trial.

After acquisition, all subjects were informed about the existence of a rule system, and were told what percentages of the items they had learned had been rule created and randomly created. They then performed the same discrimination task as in Experiment 1, except that the 28 nongrammatical items were adjusted so that item length was matched with the item lengths of the grammatical discrimination items.

Results

The mean number of trials to criterion per acquisition set showed a scattered pattern, with the different exposure groups experiencing their poorest average performance (highest mean number of trials to criterion) on different sets. In general, however, performance on the fifth set was improved over performance on the first and second sets,

Table 3
Mean Number of Trials per Acquisition Set
With 100%, 80%, 40% and 0% Grammatical Exposure

	Exposure				
Set	100%	80%	40%	0%	Set Mean
1	5.00	9.50	5.92	5.67	6.52
2	5.58	5.08	7.42	7.75	6.46
3	7.08	4.67	5.83	5.00	5.65
4	5.17	4.75	6.08	4.00	5.00
5	4.75	2.75	5.17	5.58	4.56

as can be seen by the reduced number of trials required to reach criterion on the fifth set (see Table 3). No differences were found between the four groups on number of trials per set, but the set by exposure interaction was significant ($F(12,176)=2.06$, $p<.05$). Planned comparisons for linear and curvilinear trends on the main effect for sets showed a strong linear decrease over sets in number of trials to criterion ($F(1,176)=10.76$, $p<.01$ for linear, $F(3,176)=0.16$, $p>.05$ for curvilinear).

The subjects' discrimination responses of "YES" and "NO" were scored by assigning a value of 1 (one) for each correct response (grammatical item and "YES" response or nongrammatical item and "NO" response) and a value of 0 (zero) for each incorrect response. The total percent correct on the two types of items (grammatical and nongrammatical categories) for each exposure group (100%, 80%, 40%, and 0%) are shown in Table 4. All subjects, regardless of exposure type, were better at discriminating grammatical items than at correctly identifying nongrammatical items. There is a decrease in percent correct as the amount of grammatical exposure decreases. All percent correct scores in Table 4 were significantly above chance according to t-tests.

Planned comparisons were done on the exposure groups and the exposure by category interaction. While the 80% and

Table 4
Percent Correct Scores by Category
With 100%, 80%, 40% and 0% Grammatical Exposure

	Exposure			
Category	100%	80%	40%	0%
Grammatical	76	67	63	58
Nongrammatical	70	55	57	54

40% exposure groups did not significantly differ from each other ($F(1,44)=0.02$, $p>.05$), the 100% exposure group was significantly different from the average of the 80% and 40% groups ($F(1,44)=10.94$, $p<.01$) and all three were significantly unlike the 0% exposure group ($F(1,44)=5.87$, $p<.05$). A nonorthogonal comparison of the 0% exposure group with the average of the 40% and 80% groups was not significant ($F(1,44)=1.40$, $p>.05$). None of the exposure by category interaction comparisons were significant. An analysis of variance on the data (see Appendix C for the model, mean squares and ANOVA summary table) resulted in a significant category effect ($F(1,44)=5.36$, $p<.05$), item effect ($F(54,2376)=2.68$, $p<.01$) and exposure by item interaction ($F(162,2376)=1.32$, $p<.01$).

A second analysis of variance was performed, assuming that the individual grammatical and nongrammatical items were random rather than fixed factors (see Appendix D for the model, mean squares and ANOVA summary table). Again, the purpose of this assumption was for prediction and generalization purposes; it is acknowledged that in this design the items were fixed factors. Comparisons on the exposure and exposure by category effects using this model resulted in quasi-Fs. The exposure comparisons again showed a significant difference between the 100% and the average of the 80% and 40% exposure groups ($F(1,85)=7.90$, $p<.01$) and between all three of the preceding groups and the 0% exposure group ($F(1,85)=4.34$, $p<.05$). The comparisons on the 80% group versus the 40% group and the 0% group versus the average of the 80% and 40% groups were still nonsignificant ($F(1,85)=0.24$ and $F(1,85)=1.20$ respectively, both $p>.05$). Both the item and subject main effects were significant under this analysis ($F(54,2376)=2.68$ and $F(44,2376)=3.11$ respectively, both $p<.01$). Also significant were the exposure by items and category by subjects interactions ($F(152,2376)=1.32$ and $F(44,2376)=2.70$ respectively, both $p<.01$).

Since subjects were only told that two categories existed on the discrimination task, and were not given explicit examples of either category, it is possible that a

Table 5
Percent Correct Scores for Straight Scoring and Maximizing Scoring with 100%, 80%, 40% and 0% Exposure

	Exposure			
Scoring	100%	80%	40%	0%
straight	73	61	60	56
maximizing	74	64	62	56

subject might have consistently called grammatical items nongrammatical, and vice versa. In order to allow for category confusions by the subjects, the data were rescored. Subjects were considered to have reversed categories if the number of grammatical items called grammatical (GG score) summed with the number of nongrammatical items called nongrammatical (NN score) was less than the sum of the grammatical items called nongrammatical (GN score) and nongrammatical items called grammatical (NG score). In addition, the GG and NN scores had to be less than the GN and NG scores, respectively. This eliminated the possibility of rescoring a subject who correctly categorized a large percentage of nongrammatical items but few of the grammatical items (or vice versa). There were 6 subjects who met the criteria for rescoring: one each in the 100% and 0% exposure groups, and two each in the 80% and 40% exposure groups. Their responses were rescored assuming that an answer of "YES" (grammatical) to a nongrammatical item and a "NO" (nongrammatical) response to a grammatical item were the correct categorizations. The

Table 6
Percent Correct Scores by Category
With 100%, 80%, 40% and 0% Grammatical Exposure
With Maximizing Scoring

	Exposure			
Category	100%	80%	40%	0%
Grammatical	77	68	66	57
Nongrammatical	70	60	58	56

original total percent correct for each exposure group (straight scoring) and the revised total percent correct for each exposure group (maximizing scoring) are shown in Table 5. The decrease in scores with decreased grammatical exposure is slightly more pronounced after rescoring.

Revised total percent correct on the two types of items (grammatical and nongrammatical categories) for each exposure type (100%, 80%, 40%, and 0%) are shown in Table 6. The rescored data were analyzed twice: once assuming items as a fixed factor, and once with items as a random factor. The ANOVA summary tables for these analyses can be found in Appendix E. While the category, item and exposure by item effects were slightly weaker after rescoring, the exposure main effect was much stronger. Comparisons on exposure type found the 100% scores versus the average of the 80% and 40% group scores ($F(1,44)=12.34$ and $F(1,107)=7.79$ for the fixed and random effects models, respectively) and the average of the 100%, 80% and 40% group scores versus the 0% group scores ($F(1,44)=12.38$ and $F(1,107)=7.82$ for the fixed and random effects models respectively) both to be significant at the .01 level. The comparison of the 80% group with the 40% group was still not significant ($F(1,44)=0.46$ and $F(1,107)=0.58$ for the fixed and random models, respectively, both $p>.05$). The rescored data did result in a significant difference between the 0% exposure group and the average of the 40% and 80% groups under the items as a fixed effect model ($F(1,44)=4.61$, $p<.05$), but the comparison was not significant under the items as a random effect model

($F(1,107)=3.10$, $p>.05$). The fact that this comparison is significant in only one of four analyses (rescored data, items as fixed effect) and is not significant when items are used as part of the error term (rescored data, items as random effect) makes the strength of this effect open to question. It is not clear that other subjects and/or other FSGs would result in a similar difference between the 0% exposure group and the two mixed exposure groups (80% and 40%). In addition, since the comparison in question is only just significant (the critical point for significance is 4.08), the experimenter does not feel it is justified to claim that the 80% and 40% groups could discriminate items at a significantly better rate than the 0% group. The current experimental results seem to indicate that the exposure groups with nongrammatical items (noninstances) in acquisition (80%, 40% and 0%) all performed at about the same level on the discrimination task.

Discussion

In both of the experiments presented here, exposure during acquisition that consisted of all grammatical items resulted in significantly increased ability to discriminate grammatical from nongrammatical items relative to subjects with no exposure, all nongrammatical, or mixed grammatical and nongrammatical exposure. Thus the 100% grammatical exposure subjects do seem to be able to learn something from their acquisition experience, and evidence that this is an implicit process comes largely from post-experimental subject verbalizations; no subject was able to define rules accounting for the majority of his discriminatory behavior. Implicit learning has thus been demonstrated under certain rather narrow conditions.

It should be noted that both of the preceding experiments used the same FSG. There is thus no experimental evidence to verify generalizations of these results to other FSGs. However, since both Reber (1969; Reber & Allen, 1978; Reber, et al., 1980) and Brooks (1978) have demonstrated implicit learning using other FSGs, it is probable that other complex rule systems would generate similar results. In addition, the fact that the items as random effects analyses that were performed resulted in significant exposure differences despite using items as part of the error term seems to indicate that these exposure effects are robust and not unique to the particular FSG items used in these experiments.

Learning Rates During Acquisition

While seeing all grammatical items during acquisition does seem to help discriminatory ability, it does not seem to be beneficial during acquisition. The acquisition learning rates in Experiments 1 and 2 do not support Reber's (1967) findings that the existence of a set of rules allows for a reduced number of errors over trials. In both experiments, subjects did experience a linear reduction in errors over trials, but there were no differences between the different exposure groups. Since nongrammatical (0%) exposure subjects have no available rules to extract and use to aid memorization, it seems probable that the reduction in errors is due to subjects learning how best to approach the memorization task. Subjects appear to decide on a "learn one item at a time" strategy, memorizing one item in the set first, then trying to remember two, then all three. The significant exposure by set interaction found in Experiment 2 is difficult to explain, and seems to be due mostly to the high mean number of trials (9.5) needed by the 80% exposure subjects on their first acquisition set (possible Type I error).

The Importance of Control Groups

While the amount of exposure to rule created items does not seem to affect learning rates, it does affect later performance at identifying novel grammatical items. The existence of implicit learning is supported by the significant increase in discriminatory ability shown by the grammatical exposure subjects in Experiment 1. However, both the no exposure and nongrammatical exposure group subjects performed at a rate above chance, implying that Reber's criterion of significance above chance (50%) is an incorrect baseline.

The possibility that 0% exposure subjects in Experiment 2 scored above chance on the discrimination task because they were implicitly absorbing the rules during the discrimination task was investigated in a post-hoc analysis of their first half and second half scores. If learning did occur during the discrimination task, then scores should improve with time and their second half scores should be better than their first half scores. No significant difference was found between first and second half scores (mean percent correct of 55% and 57% respectively, $t(22)=0.48$, two-tailed, $p>.05$). Rescoring of the 0% subject responses to give the maximum possible scores (see page 43) still did not result in a difference between first and second half scores on the discrimination task (mean percent correct of 55% and 58% respectively, $t(22)=0.63$, two-tailed, $p>.05$). On the basis of these results, it is not possible to argue that the subjects were implicitly absorbing the rules during the discrimination task, as this should result in better second half scores. While Reber (1967) did find evidence of learning during the discrimination phase, his subjects classified each discrimination item twice and thus had a greater amount of viewing time both per item and overall. Subjects in Experiment 2 did not have this added time benefit and did not exhibit behavior in line with abstraction of the rules.

Since the above chance performance of the control groups appears not to be due to learning during the discrimination task, subject response patterns on the individual discrimination items were intuitively examined for clues to the subjects' discriminatory behavior. Examination of the individual strings and average exposure group responses to each string in Experiment 1 showed that nongrammatical and no exposure subjects did as well as grammatical subjects on grammatical items that "looked regular", such as "TXXTTVV" and "PTTVV" (see Appendix F). The control group subjects were also good at correctly identifying "irregular" nongrammatical items such as "XPVS" and "PVVTSP". It appears that these subjects assumed a regular letter pattern (e.g. single letter, double letters, double letters) was indicative of rules, and irregular patterns of random generation.

Experiment 2 results supported the hypothesis that subjects with predominantly nongrammatical exposure use a "regular pattern" approach in identifying items (see Appendix G). The 0% and 40% exposure groups were better than the 100% and 80% exposure groups at identifying regularly patterned "TXXT..." strings as grammatical. The fact that the "TXXT" pattern was not found in any acquisition item helps to account for the poorer performance of the 100% and 80% exposure groups, but does not explain the 40% and 0% performance. The latter's decision making processes become more obvious when it is noted that 40% and 0% exposure subjects were not better at classifying "TXXT..VPS" patterns as grammatical. These patterns do not follow a regular, mathematical pattern of letters, whereas strings such as "TXXTVV" and "TXXTTTVV" do have a mathematical pattern (single-double-single-double and single-double-triple-double patterns respectively).

Detecting pattern regularity or the lack thereof also may have helped the 0% and 40% subjects identify certain nongrammatical items correctly at a higher rate than the 100% and 80% exposure subjects. Mathematically irregular nongrammatical items, which began with P or T (the two FSG allowable initial letters) and contained a few FSG allowable bigrams (such as VV), fooled the subjects given greater grammatical exposure (100% and 80%) into identifying them as grammatical items. The 40% and 0% subjects, who appeared to be using a different criterion, were more likely to correctly classify these items as nongrammatical, possibly because of their of "lack of regularity."

Further evidence that subjects did not always make discrimination decisions by using only FSG rules comes from an examination of those items which 100% exposure subjects correctly classified at a higher rate than all other subjects. The majority of these items contain "VPX" or "VPS". Since these trigrams are not easily recognizable as grammatical by mathematical pattern rules, grammatical items containing them would be less likely to be correctly identified by less informed subjects (40%, and 0% exposure groups) who were using mathematical (regularity) heuristics.

Subjects with little or no previous rule exposure (nongrammatical, no exposure, 40% and 0% groups) thus appear able to correctly detect some grammatical and nongrammatical items on the basis of a mathematical "regularity" pattern heuristic. While the subjects' knowledge of probability and pattern rules is not completely accurate (see Nisbett & Ross, 1980, chapters 4 and 5, for analyses of subject perceptions of randomness), even a partially correct heuristic allows subjects to discriminate some FSG items, since by definition acceptable grammatical items have probabilistic intra-relationships (Reber, 1967).

While the above examination of the individual strings and the conclusions about subject behavior are all intuitively based, the fact remains that both experiments showed a significant exposure by item interaction. This indicates that subjects with different types of acquisition experience will respond to the individual discrimination strings in different ways. These response differences may not only be due to learning which occurred during acquisition, but also to knowledge about patterns and their formation possessed by the subjects prior to the experiment. Subjects in Allen and Reber's (1980) two year follow-up study may have scored above chance on the discrimination task after a two year lag not because they remembered the rules but because of simple guessing based on regular patterns and probability rules.

Reber might argue that there is a key difference between his work and the current studies. Nongrammatical items in the current experiments were randomly created, whereas Reber's NG items were single letter FSG violations. Reber could propose that discriminating single letter FSG violations from the grammatical items requires a detailed knowledge of the rules. Uninformed subjects should be unable to perform at above chance levels simply because both the grammatical and nongrammatical items will be very similar in appearance. However, there is no way of knowing whether changing a single letter in a grammatical item (making it nongrammatical) distorts the "regularity" of the item and thus makes it more likely to be called nongrammatical by uninformed subjects. While Reber may be right in assuming that chance is an accurate baseline for his experiments, he has not shown experimental evidence that the performance levels attained by his grammatical exposure subjects when making "fine" discriminations are beyond the performance levels possible for subjects without grammatical exposure. Without the proper controls to set a systematic guessing baseline, the percentage of correct responses on the discrimination task due to implicit learning is unclear in Reber's experiments.

It should be noted that one cannot find an exposure by item interaction when the experiment has only one exposure group, as in Reber's studies. This is important, because the differing behavior on individual items from subjects with different acquisition exposure provides important clues to subject decision strategies. The significant exposure by item interaction also accounts for other significant effects which occur when using this learning paradigm, such as the significant item main effect; if subjects in a certain exposure group have a higher probability of correctly classifying certain items and a lower probability for other items, the overall item averages may well differ. The exposure by item interaction (items as fixed effect) can also account for the occasionally significant category main effect and category by exposure interactions found in Experiments 1 and 2, since strings are confounded with categories in this experimental design.

Nongrammatical Exposure and Implicit Learning

The effect of acquisition exposure to noninstances on later discriminatory ability can be seen in results from Experiment 2. The 80% and 40% exposure groups were in general not able to make significantly better discriminations than the 0% exposure group, indicating that the former were not able to make use of their rule exposure during acquisition. While analysis of the rescored data with items as a fixed effect refutes this position, the differences between the 0% and the average of the 40% and 80% exposure groups is not strongly significant and is not significant when items are used as an error term under the items as a random effect model. This fact implies that the presence of malformed or erroneous instances is detrimental to implicit learning under the observational/memorization learning paradigm. In general, it appears that only the 100% grammatical exposure group was able to make use of the rules in any significant way. It is unfortunately not possible in this experimental design to determine whether the lessened discriminatory ability of the mixed and nongrammatical exposure groups is due to an insufficient number of representative grammatical items or to the presence of noninstances which might interfere with correct rule abstraction, since the number of grammatical acquisition items is confounded with the number of nongrammatical acquisition items.

It is also unclear to what extent the 100% exposure group's increased ability to discriminate grammatical and nongrammatical items is due to a larger potential set of exemplars for analogies rather than to a better abstract representation of the rules. Examination of the significant exposure by item interaction shows that the 100% group does better on grammatical items which are moderately analogous to at least half of the grammatical acquisition items (e.g. posses similar trigrams such as VPS and VPX). The greater the number of representative grammatical acquisition items seen, the greater the number of available exemplars and the greater the salience of the rules (Reber, et al., 1980). Hence the significant exposure by item interaction supports both the analogy theory and the implicit abstraction theory of implicit learning.

Similarities in Problem Solving Literature

While it is not possible to isolate the learning process involved in the observational/memorization learning paradigm, studies from the problem solving literature do provide some insight into the analog and implicit abstraction processes and may be useful when designing an implicit learning experiment to distinguish between the analog and implicit abstraction theories. In an experiment by Chi, Feltovich and Glaser (1981) physics experts (professors) and novices (completed one physics course) first sorted physics problems into categories and then verbalized while solving them. It was found that novices tend to categorize the problems based on surface details such as "fulcrum used", "spring tension", or "work related". Experts categorized the problems according to the three main laws of physics. The experts, having greater experience with this type of problem, were able to categorize the problems on the basis of rules, whereas the novices, lacking the necessary amount of experience, concentrated on individual items. The difference between novices and experts categorizations was thus due not to knowledge of the rules (the novices did know the three main laws of physics) but to quick identification of the useful rules. It appears that experts may be using an implicit strategy when comparing problems to their rule knowledge base, while novices may be using an analog strategy of comparing problem details with remembered problems.

This analog/rule difference in approach between experts and novices was also found in a study by Larkin, McDermott, Simon and Simon (1980). They discovered that when solving physics problems, novices often work backwards in a bottom-up style, whereas experts appear to use a more top-down approach by working forward from the givens to the answers. Novices seem to be using a "search and identify" strategy which may or may not lead to a correct solution. If the similarity between the problem and remembered exemplar problems is not strong enough, the novices will be less likely to discover the correct method of solution through analogy. The "flash of insight" which leads to solution of an obscure problem may be due to implicit abstraction! Experts begin with a more wholistic grasp of the problem, possibly due not only to their greater knowledge base but to their implicit approach.

Anderson, Greeno, Kline and Neves (1981) theorized that problem solving starts by using analogy to stored exemplars to locate examples of similar problems. As a result of increased experience with analogous problems, generalization occurs and global rules for solution are developed (Fried & Holyoak, 1983). These abstract rule structures may also be developed by induction, provided heuristics are used to adequately constrain the process (Anderson, et al., 1981; Pinker, 1979). Over time, a schema for correct action under certain circumstances is developed. While it is not known how much of this process is implicit, indications are that an interaction between explicit and implicit learning can be beneficial. Subjects who are first taught a FSG and then experience implicit learning of grammatical items outperform subjects who experience unembellished implicit learning or simple FSG learning without item experience (Reber, et al., 1980). One hypothesis is that early FSG training acts to establish cognitive boundaries for implicit rule induction, allowing subjects to make better use of their item observation time.

The idea of a schema of rules for problem solving seems to imply that once rules are learned, individual problem exemplars no longer serve a purpose and are forgotten. However, it is not beneficial for individuals to learn only exceptionless rules (Medin & Schaffer, 1978). Since category membership is not always all or none (e.g. "man is ape and not ape"), specific instances are helpful in identifying the range of acceptable deviations from the prototype or rules (Jones, 1982). In some instances, such as in chess, the entire rule system must yield to the exceptional instance. Brooks (1978) calls this cognitive odds playing. Knowledge of the particular can overrule the general, and the general can be used to critically examine the particular for category membership.

Evidence for the interaction of exemplar and rule knowledge comes from a study on computer programming experts and novices (McKeithen, Reitman, Rueter & Hirtle, 1981). Subjects were asked to learn a list of one syllable words taken from a computer programming language by sorting the words into related groups. When the subjects felt that they knew the list, they were asked to recall it without reference to the sorted words. Novices (one programming course) and expert programmers (graduate students or professors) had the same amount of organization in their recall clustering, but experts were consistently better at cued recall of the words than the novices. Since novices clustered the words in mini-sentences, and experts in terms of programming concepts, one hypothesis for their differential performance is based on the analogy theory. A given word may be analogous to many other stored sentence exemplars, but to only a few programming concept exemplars. Thus experts would be less likely to cue into erroneous items and should make fewer errors during recall. The experts' more detailed knowledge of programming languages and syntax rules allowed them to efficiently cluster the exemplars.

Thus, having both types of information available can greatly speed reaction to stimuli and increase recall accuracy. Storage of exemplars allows for quick identification of appropriate categories or rule systems via analogy (Anzai & Simon, 1977; Green, 1979). The selected rule system is then used to quickly guide or generate a response (Larkin, et al., 1980; Winston, 1978).

It is interesting to note that most of the preceding theories on problem solving and category learning presume that exemplar learning (analogy theory) precedes rule learning (implicit abstraction theory). Rule information is developed through repeated exposure to exemplars (Gibson, 1979; Grandy, 1972). Because the contingencies of experience are variable and continuously changing for each individual, many of the rules known will be difficult to pinpoint and to verbalize (Baldwin & Baldwin, 1978). In this sense, much of our knowledge is implicit, and verbalizable knowledge is a special case (MacKay, 1974).

Given that nonverbalizable learning is indeed a major component of human learning, it must be asked whether Reber's implicit abstraction theory truly models natural nonverbalizable learning. Much of the problem solving literature contains examples of both implicit abstraction and exemplar learning in fairly naturalistic and ecologically valid situations. None of the problem solving experiments show evidence of only one type of learning. For example, expert physics subjects still used exemplar knowledge to guide and fine tune their implicit rule search (Chi, Feltovich, & Glaser, 1981). Thus, if Reber's paradigm is valid, it is unique in modeling human learning wherein only one type of learning (implicit abstraction) occurs. Further studies need to be done to determine if subjects learning under Reber's paradigm do indeed bypass exemplar learning completely, as Reber claims. The possibility that they learn exemplars and then move on to rule abstraction can not as yet be discounted.

Summary

The major findings of these two experiments are that chance is not a proper baseline for implicit learning studies, and that the amount of grammatical and nongrammatical exposure determines the level of discriminatory ability and differentially affects responses to individual items. While implicit learning does occur, other factors such as prior knowledge may influence performance on the discrimination tasks designed to test the extent of subjects' implicitly acquired rule knowledge. Control groups are essential in isolating the degree of discriminatory performance due to factors other than implicit learning during the acquisition phase.

The results support Reber's contentions that implicit learning is a recognizable learning process. His learning paradigm has been shown to produce significant results using control group baselines. However, the results from Experiments 1 and 2 also call into question evidence on the strength and longevity of implicit learning. Subjects shown noninstances of the grammar (nongrammatical items) as well as grammatical items during acquisition do not seem to be able to use the grammatical rule information when making discriminations. It is not clear whether this is due to an inability to discount the nongrammatical items when abstracting the rules, or to an insufficient number of rule created items for study. An explanation based on the analogy theory is also potentially valid. Subjects could be distinguishing between the two types of stored exemplars on the basis of heuristics (e.g. irregularity/regularity) which inadequately reflect the grammar rules. This would lead to errors in judgement when making analogies to new items, since the exemplar category assignments would not be totally accurate. Further work thus needs to be done to determine whether the acquisition learning process is all analog storage of exemplars, all implicit abstraction of the rules, or whether it begins with analogies and proceeds to rule induction, as suggested by problem solving literature.

All future work should make use of appropriate control groups. It is recommended that a random exposure (nongrammatical or 0%) control group be used in future implicit learning studies. While both control groups in Experiment 1 (random exposure or no exposure) exhibited similar response behavior and were not statistically different, the former controls for both lack of exposure to the rules and participation in a memorization task with nonmeaningful stimulus items.

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Appendix A
Experiment 1 - Analysis 1

Model I

	Source	Effect	Level	Random/Fixed (nesting)
a	exposure	α_j	j = 1,2,3	F
b	category	β_k	k = 1,2	F
c	strings	$\gamma_{l(k)}$	l = 1...28	F (category)
s	subjects	$s_{i(j)}$	i = 1...17	R (exposure)

Expected Mean Squares[1]	df	F
a + s	2	MSa/MSs
b + bs	1	MSb/MSbs
c + cs	54	MSc/MScs
s	48	------
ab + bs	2	MSab/MSbs
ac + cs	108	MSac/MScs
bs	48	------
cs	2,592	------
	2,855 = N-1	

ANOVA Summary Table

	SS	df	MS	F
a	19.6912	2	9.8456	see comparisons
b	1.3029	1	1.3029	F(1,48) = 3.6857
c	44.2066	54	.8186	F(54,2592) = 4.0706 **
s	22.3592	48	.4658	
ab	2.425	2	1.2125	see comparisons
ac	30.7794	108	.2850	F(108,2592) = 1.4172 *
bs	16.9686	48	.3535	
cs	521.2604	2592	.2011	

* $p < .05$
** $p < .01$

Appendix B
Experiment 1 - Analysis 2

Model II

Source	Effect	Level	Random/Fixed (nesting)
a exposure	α_j	j = 1,2,3	F
b category	β_k	k = 1,2	F
c strings	$c_{l(k)}$	l = 1...28	R (category)
s subjects	$s_{i(j)}$	i = 1...17	R (exposure)

Expected Mean Squares[1]	df	F
a + s + ac + cs	2	MSa + MScs/MSs + MSac
b + c + bs + cs	1	MSb + MScs/MSc + MSbs
c + cs	54	MSc/MScs
s + cs	48	MSs/MScs
ab + ac + bs + cs	2	MSab + MScs/MSac + MSbs
ac + cs	108	MSac/MScs
bs + cs	48	MSbs/MScs
cs	2,592	------
	2,855 = N-1	

ANOVA Summary Table

	SS	df	MS	F
a	19.6912	2	9.8456	see comparisons
b	1.3029	1	1.3029	$F(1,92) = 1.2832$
c	44.2066	54	.8186	$F(54,2592) = 4.0706$ **
s	22.3592	48	.4658	$F(48,2592) = 2.3163$ **
ab	2.425	2	1.2125	see comparisons
ac	30.7794	108	.2850	$F(108,2592) = 1.4172$ *
bs	16.9686	48	.3535	$F(48,2592) = 1.7578$ **
cs	521.2604	2592	.2011	

* $p < .05$
** $p < .01$

Appendix C
Experiment 2 - Anaylsis 1

Model I

Source	Effect	Level	Random/Fixed (nesting)
a exposure	α_j	j = 1,2,3,4	F
b category	β_k	k = 1,2	F
c strings	$\gamma_{l(k)}$	l = 1...28	F (category)
s subjects	$s_{i(j)}$	i = 1...12	R (exposure)

Expected Mean Squares[1]	df	F
a + s	3	MSa/MSs
b + bs	1	MSb/MSbs
c + cs	54	MSc/MScs
s	44	------
ab + bs	3	MSab/MSbs
ac + cs	162	MSac/MScs
bs	44	------
cs	2,376	------
	2,687 = N-1	

ANOVA Summary Table

	SS	df	MS	F
a	10.7664	3	3.5888	see comparisons
b	3.4286	1	3.4286	F(1,44) = 5.3572 *
c	29.6949	54	.5499	F(54,2376) = 2.6759 **
s	28.1607	44	.6400	
ab	.6964	3	.2321	see comparisons
ac	44.0789	162	.2721	F(162,2376) = 1.3241 **
bs	24.3750	44	.5540	
cs	488.2976	2376	.2055	

* $p < .05$
** $p < .01$

Appendix D
Experiment 2 - Analysis 2

Model II

	Source	Effect	Level	Random/Fixed (nesting)
a	exposure	α_j	j = 1,2,3,4	F
b	category	β_k	k = 1,2	F
c	strings	$c_{l(k)}$	l = 1...28	R (category)
s	subjects	$s_{i(j)}$	i = 1...12	R (exposure)

Expected Mean Squares[1]	df	F
a + s + ac + cs	3	MSa + MScs/MSs + MSac
b + c + bs + cs	1	MSb + MScs/MSc + MSbs
c + cs	54	MSc/MScs
s + cs	44	MSs/MScs
ab + ac + bs + cs	3	MSab + MScs/MSac + MSbs
ac + cs	162	MSac/MScs
bs + cs	44	MSbs/MScs
cs	2,376	------
	2,687 = N-1	

ANOVA Summary Table

	SS	df	MS	F
a	10.7664	3	3.5888	see comparisons
b	3.4286	1	3.4286	F(1,97) = 3.2921
c	29.6949	54	.5499	F(54,2376) = 2.6759 **
s	28.1607	44	.6400	F(44,2376) = 3.1144 **
ab	.6964	3	.2321	see comparisons
ac	44.0789	162	.2721	F(162,2376) = 1.3241 **
bs	24.3750	44	.5540	F(44,2376) = 2.6959 **
cs	488.2976	2376	.2055	

* $p < .05$
** $p < .01$

Appendix E
Experiment 2 - Rescored Data ANOVAs

ANOVA Summary Table
(Model I - Items as fixed effect)

	SS	df	MS	F
a	10.4941	3	3.4980	see comparisons
b	2.5015	1	2.5015	F(1,44) = 4.3152 *
c	23.6950	54	.4388	F(54,2376) = 2.1076 **
s	18.3393	44	.4168	
ab	.4925	3	.1642	see comparisons
ac	43.7217	162	.2699	F(162,2376) = 1.2963 *
bs	25.5060	44	.5797	
cs	494.6547	2376	.2082	

ANOVA Summary Table
(Model II - Items as random effect)

	SS	df	MS	F
a	10.4941	3	3.4980	see comparisons
b	2.5015	1	2.5015	F(1,93) = 2.6605
c	23.6950	54	.4388	F(54,2376) = 2.1076 **
s	18.3393	44	.4168	F(44,2376) = 2.0019 **
ab	.4925	3	.1642	see comparisons
ac	43.7217	162	.2699	F(162,2376) = 1.2963 *
bs	25.5060	44	.5797	F(44,2376) = 2.7843
cs	494.6547	2376	.2082	

Key to Letter Meanings

a = exposure
b = category
c = strings
s = subjects

* $p < .05$
** $p < .01$

Appendix F
Percent Correct
For the Exposure by Discrimination String Interaction
In Experiment 1

		Exposure	
Strings	Grammatical	Nongrammatical	Zero
Grammatical			
PVPS	71	47	59
PTVV	76	65	41
PTTVV	94	76	88
TSSXS	88	47	65
TXXVV	82	76	71
PTTVPS	88	47	47
TSXXVV	88	47	94
TXXTVV	82	82	88
TXXVPS	88	47	53
PTTTTVV	94	59	65
PTVPXVV	76	18	24
PVPXTVV	82	65	35
TSSSSXS	94	47	41
TSXXTVV	88	65	65
TXXTTVV	76	82	88
TXXTVPS	76	41	41
PTTTTTVV	76	59	65
PTTTTVPS	82	47	53
PTTVPXVV	76	35	35
PTVPXVPS	76	29	47
PVPXTTVV	82	53	59
TSSSXXVV	82	82	76
TSSXXTVV	82	82	94
TSSXXVPS	82	59	53
TSXXTTVV	76	76	82
TSXXTVPS	82	24	41
TXXTTTVV	76	65	82
TXXTTVPS	71	53	47

(continued on next page)

		Exposure	
Strings	Grammatical	Nongrammatical	Zero
Nongrammatical			
PPS	47	65	29
PSP	71	35	53
XSP	76	53	71
TSX	59	65	71
TVST	76	59	65
XPVS	76	76	82
XVTS	71	59	65
XXVX	71	76	59
VSSXX	71	41	47
SPPVP	88	71	88
SSSVT	71	59	82
VPVTT	53	59	71
TSTXT	65	29	29
VSPSV	59	29	35
PVSXVV	59	76	53
PVVTSP	53	76	71
TXXVSS	41	41	18
VPVPSV	65	35	47
XSXXPTS	65	71	59
PSPSXSP	88	53	47
SXXPSTV	94	76	88
XSPPTPS	100	59	76
STTPSVP	82	82	76
TVXVVVS	47	59	65
SPTVPTP	76	47	47
PTSPXXVX	82	59	59
VVPTPSST	82	41	29
VXXPPVXS	65	71	53

		Exposure	
Strings	Grammatical	Nongrammatical	Zero
Nongrammatical			
PPS	47	65	29
PSP	71	35	53
XSP	76	53	71
TSX	59	65	71
TVST	76	59	65
XPVS	76	76	82
XVTS	71	59	65
XXVX	71	76	59
VSSXX	71	41	47
SPPVP	88	71	88
SSSVT	71	59	82
VPVTT	53	59	71
TSTXT	65	29	29
VSPSV	59	29	35
PVSXVV	59	76	53
PVVTSP	53	76	71
TXXVSS	41	41	18
VPVPSV	65	35	47
XSXXPTS	65	71	59
PSPSXSP	88	53	47
SXXPSTV	94	76	88
XSPPTPS	100	59	76
STTPSVP	82	82	76
TVXVVVS	47	59	65
SPTVPTP	76	47	47
PTSPXXVX	82	59	59
VVPTPSST	82	41	29
VXXPPVXS	65	71	53

Appendix G
Percent Correct
For the Exposure by Discrimination String Interaction
In Experiment 2

	Exposure			
Strings	100%	80%	40%	0%
Grammatical				
PVPS	100	75	58	42
PTVV	83	50	67	83
PTTVV	75	75	83	67
TSSXS	92	75	58	42
TXXVV	100	58	75	58
PTTVPS	92	67	58	42
TSXXVV	100	83	75	58
TXXTVV	50	50	75	67
TXXVPS	92	75	67	75
PTTTTVV	33	83	58	42
PTVPXVV	75	75	42	42
PVPXTVV	100	67	33	42
TSSSSXS	92	75	75	42
TSXXTVV	67	92	75	75
TXXTTVV	50	67	92	67
TXXTVPS	58	67	50	58
PTTTTTVV	67	58	75	42
PTTTTVPS	83	75	75	50
PTTVPXVV	67	42	33	75
PTVPXVPS	92	58	42	33
PVPXTTVV	75	67	92	75
TSSSXXVV	75	92	92	58
TSSXXTVV	92	58	75	50
TSSXXVPS	100	83	83	83
TSXXTTVV	75	50	75	50
TSXXTVPS	83	67	58	75
TXXTTTVV	42	42	50	58
TXXTTVPS	50	83	50	50

(continued on next page)

	Exposure			
Strings	100%	80%	40%	0%
Nongrammatical				
TVST	75	58	92	67
XXVX	83	67	42	67
VSSXX	58	50	42	25
SSSVT	75	67	50	100
VSPSV	58	50	75	50
PVSXVV	33	58	75	75
PVVTSP	58	42	67	58
TXXVSS	50	67	17	42
VPVPSV	58	25	58	42
XSXXPTS	67	58	33	50
PSPSXSP	92	75	58	50
SXXPSTV	92	92	50	50
XSPPTPS	75	83	67	67
STTPSVP	67	50	58	33
TVXVVVS	50	58	67	50
SPTVPTP	92	67	50	67
PTSPXXVX	83	75	83	50
PXSTXVVS	67	58	75	50
TXTSPSXV	83	75	75	75
TSXTPPTV	67	75	50	67
TVVSVVVX	83	75	50	67
VSSSVXST	67	42	67	58
TPVSVPXP	67	75	42	75
VVXTTPPT	75	50	42	50
TVPPPSSS	42	25	50	58
VPXTTPVV	67	58	67	33
VVPTPSST	92	58	58	42
VXXPPVXS	92	50	58	42

Appendix H
Vita

PERSONAL INFORMATION

Nancy Eleanor Boylston Rudzki
1659 Millard Street
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EDUCATION

Lehigh University
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area: general experimental psychology
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degree: M.S. in October, 1983

Duke University
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degree: B.S. in computer science in May, 1980
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WORK EXPERIENCE

9/81-pres Moravian College
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9/81-12/82 Lehigh University
location: Bethlehem, PA. 18015
job: teaching assistant for statistics and introductory psychology seminars

1/81-6/81 Northampton County Community College
location: Bethlehem, PA. 18017
job: adjunct professor, taught "Introduction to Data Processing"

6/80-6/81 Western Electric Corporation
location: Piscataway, N.J. 08854
job: UNIX operating system consulting and documentation (Customer Service Dept.)

5/79-8/79 Burroughs Corporation
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job: systems analyst and computer programmer in business information systems

Footnote

[1]The expected mean squares are presented in simplified form: (1) small letters represent components of variance usually denoted as σ^2 with these letters as subscripts, and (2) the numerical coefficients that multiply the components of variance are omitted. For example, ab stands for $JK\sigma^2_{ab}$.